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Do Drugs Have Religious Import?: HUSTON SMITH 517

An Empirical Basis for Psychological Realism: MICHAEL AN-
THONY SLOTE 530

Book Review

LOUIS GOTTSCHALK, ed.: *Generalization in the Writing of History:*
LOUIS O. MINK 538

New Anthologies, Classical and Contemporary 543

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THE JOURNAL OF PHILOSOPHY

DO DRUGS HAVE RELIGIOUS IMPORT? *

UNTIL six months ago, if I picked up my phone in the Cambridge area and dialed KISS-BIG, a voice would answer, "If-if." These were coincidences: KISS-BIG happened to be the letter equivalents of an arbitrarily assigned telephone number, and I.F.I.F. represented the initials of an organization with the improbable name of the International Federation for Internal Freedom. But the coincidences were apposite to the point of being poetic. "Kiss big" caught the euphoric, manic, life-embracing attitude that characterized this most publicized of the organizations formed to explore the newly synthesized consciousness-changing substances; the organization itself was surely one of the "iffy-est" phenomena to appear on our social and intellectual scene in some time. It produced the first firings in Harvard's history, an ultimatum to get out of Mexico in five days, and "the miracle of Marsh Chapel," in which, during a two-and-one-half-hour Good Friday service, ten theological students and professors ingested psilocybin and were visited by what they generally reported to be the deepest religious experiences of their lives.

Despite the last of these phenomena and its numerous if less dramatic parallels, students of religion appear by and large to be dismissing the psychedelic drugs that have sprung to our attention in the '60s as having little religious relevance. The position taken in one of the most forward-looking volumes of theological essays to have appeared in recent years—*Soundings*, edited by A. R. Vidler¹—accepts R. C. Zaehner's *Mysticism Sacred and Profane* as having "fully examined and refuted" the religious claims for mescaline which Aldous Huxley sketched in *The Doors of Perception*. This closing of the case strikes me as premature, for it looks as if the drugs have light to throw on the history of religion,

* The emended version of a paper presented to The Woodrow Wilson Society, Princeton University, on May 16, 1964.

¹ *Soundings: Essays concerning Christian Understandings*, A. R. Vidler, ed. (Cambridge: University Press, 1962). The statement cited appears on page 72, in H. A. Williams's essay on "Theology and Self-awareness."

the phenomenology of religion, the philosophy of religion, and the practice of the religious life itself.

1. *Drugs and Religion Viewed Historically*

In his trial-and-error life explorations man almost everywhere has stumbled upon connections between vegetables (eaten or brewed) and actions (yogi breathing exercises, whirling-dervish dances, flagellations) that alter states of consciousness. From the psychopharmacological standpoint we now understand these states to be the products of changes in brain chemistry. From the sociological perspective we see that they tend to be connected in some way with religion. If we discount the wine used in Christian communion services, the instances closest to us in time and space are the peyote of The Native American [Indian] Church and Mexico's 2000-year-old "sacred mushrooms," the latter rendered in Aztec as "God's Flesh"—striking parallel to "the body of our Lord" in the Christian eucharist. Beyond these neighboring instances lie the *soma* of the Hindus, the *haoma* and hemp of the Zoroastrians, the Dionysus of the Greeks who "everywhere . . . taught men the culture of the vine and the mysteries of his worship and everywhere [was] accepted as a god,"² the *benzoin* of Southeast Asia, Zen's tea whose fifth cup purifies and whose sixth "calls to the realm of the immortals,"³ the *pituri* of the Australian aborigines, and probably the mystic *kykeon* that was eaten and drunk at the climactic close of the sixth day of the Eleusinian mysteries.⁴ There is no need to extend the list, as a reasonably complete account is available in Philippe de Félice's comprehensive study of the subject, *Poisons sacrés, ivresses divines*.

More interesting than the fact that consciousness-changing devices have been linked with religion is the possibility that they actually initiated many of the religious perspectives which, taking root in history, continued after their psychedelic origins were forgotten. Bergson saw the first movement of Hindus and Greeks toward "dynamic religion" as associated with the "divine rapture" found in intoxicating beverages;⁵ more recently Robert Graves, Gordon Wasson, and Alan Watts have suggested that most religions arose from such chemically induced theophanies. Mary

² Edith Hamilton, *Mythology* (New York: Mentor, 1953), p. 55.

³ Quoted in Alan Watts, *The Spirit of Zen* (New York: Grove Press, 1958), p. 110.

⁴ George Mylonas, *Eleusis and the Eleusinian Mysteries* (Princeton, N.J.: Princeton Univ. Press, 1961), p. 284.

⁵ *Two Sources of Morality and Religion* (New York: Holt, 1935), pp. 206-212.

Barnard is the most explicit proponent of this thesis. "Which . . . was more likely to happen first," she asks,⁶ "the spontaneously generated idea of an afterlife in which the disembodied soul, liberated from the restrictions of time and space, experiences eternal bliss, or the accidental discovery of hallucinogenic plants that give a sense of euphoria, dislocate the center of consciousness, and distort time and space, making them balloon outward in greatly expanded vistas?" Her own answer is that "the [latter] experience might have had . . . an almost explosive effect on the largely dormant minds of men, causing them to think of things they had never thought of before. This, if you like, is direct revelation." Her use of the subjunctive "might" renders this formulation of her answer equivocal, but she concludes her essay on a note that is completely unequivocal: "Looking at the matter coldly, unintoxicated and unentranced, I am willing to prophesy that fifty theobotanists working for fifty years would make the current theories concerning the origins of much mythology and theology as out-of-date as pre-Copernican astronomy."

This is an important hypothesis—one which must surely engage the attention of historians of religion for some time to come. But as I am concerned here only to spot the points at which the drugs erupt onto the field of serious religious study, not to ride the geysers to whatever heights, I shall not pursue Miss Barnard's thesis. Having located what appears to be the crux of the historical question, namely the extent to which drugs not merely duplicate or simulate theologically sponsored experiences but generate or shape theologies themselves, I turn to phenomenology.

2. *Drugs and Religion Viewed Phenomenologically*

Phenomenology attempts a careful description of human experience. The question the drugs pose for the phenomenology of religion, therefore, is whether the experiences they induce differ from religious experiences reached naturally, and if so how.

Even the Bible notes that chemically induced psychic states bear *some* resemblance to religious ones. Peter had to appeal to a circumstantial criterion—the early hour of the day—to defend those who were caught up in the Pentecostal experience against the charge that they were merely drunk: "These men are not drunk, as you suppose, since it is only the third hour of the day" (Acts 2:15); and Paul initiates the comparison when he admonishes the Ephesians not to "get drunk with wine . . . but [to] be filled

⁶ "The God in the Flowerpot," *The American Scholar* 32, 4 (Autumn, 1963): 584, 586.

with the spirit" (Ephesians 5:18). Are such comparisons, paralleled in the accounts of virtually every religion, superficial? How far can they be pushed?

Not all the way, students of religion have thus far insisted. With respect to the new drugs, Prof. R. C. Zaehner has drawn the line emphatically. "The importance of Huxley's *Doors of Perception*," he writes, "is that in it the author clearly makes the claim that what he experienced under the influence of mescaline is closely comparable to a genuine mystical experience. If he is right, . . . the conclusions . . . are alarming."⁷ Zaehner thinks that Huxley is not right, but I fear that it is Zaehner who is mistaken.

There are, of course, innumerable drug experiences that have no religious feature; they can be sensual as readily as spiritual, trivial as readily as transforming, capricious as readily as sacramental. If there is one point about which every student of the drugs agrees, it is that there is no such thing as the drug experience *per se*—no experience that the drugs, as it were, merely secrete. Every experience is a mix of three ingredients: drug, set (the psychological make-up of the individual), and setting (the social and physical environment in which it is taken). But given the right set and setting, the drugs can induce religious experiences indistinguishable from experiences that occur spontaneously. Nor need set and setting be exceptional. The way the statistics are currently running, it looks as if from one-fourth to one-third of the general population will have religious experiences if they take the drugs under naturalistic conditions, meaning by this conditions in which the researcher supports the subject but does not try to influence the direction his experience will take. Among subjects who have strong religious inclinations to begin with, the proportion of those having religious experiences jumps to three-fourths. If they take the drugs in settings that are religious too, the ratio soars to nine in ten.

How do we know that the experiences these people have really are religious? We can begin with the fact that they say they are. The "one-fourth to one-third of the general population" figure is drawn from two sources. Ten months after they had had their experiences, 24 per cent of the 194 subjects in a study by the California psychiatrist Oscar Janiger characterized their experiences as having been religious.⁸ Thirty-two per cent of the 74

⁷ *Mysticism, Sacred and Profane* (New York: Oxford, 1961), p. 12.

⁸ Quoted in William H. McGlothlin, "Long-lasting Effects of LSD on Certain Attitudes in Normals," printed for private distribution by the RAND Corporation, May, 1962, p. 16.

subjects in Ditman and Hayman's study reported, looking back on their LSD experience, that it looked as if it had been "very much" or "quite a bit" a religious experience; 42 per cent checked as true the statement that they "were left with a greater awareness of God, or a higher power, or ultimate reality."⁹ The statement that three-fourths of subjects having religious "sets" will have religious experiences comes from the reports of sixty-nine religious professionals who took the drugs while the Harvard project was in progress.¹⁰

In the absence of (a) a single definition of religious experience acceptable to psychologists of religion generally and (b) fool-proof ways of ascertaining whether actual experiences exemplify any definition, I am not sure there is any better way of telling whether the experiences of the 333 men and women involved in the above studies were religious than by noting whether they seemed so to them. But if more rigorous methods are preferred, they exist; they have been utilized, and they confirm the conviction of the man in the street that drug experiences can indeed be religious. In his doctoral study at Harvard University, Walter Pahnke worked out a typology of religious experience (in this instance of the mystical variety) based on the classic cases of mystical experiences as summarized in Walter Stace's *Mysticism and Philosophy*. He then administered psilocybin to ten theology students and professors in the setting of a Good Friday service. The drug was given "double-blind," meaning that neither Dr. Pahnke nor his subjects knew which ten were getting psilocybin and which ten placebos to constitute a control group. Subsequently the reports the subjects wrote of their experiences were laid successively before three college-graduate housewives who, without being informed about the nature of the study, were asked to rate each statement as to the degree (strong, moderate, slight, or none) to which it exemplified each of the nine traits of mystical experience enumerated in the typology of mysticism worked out in advance. When the test of significance was applied to their statistics, it showed that "those subjects who received psilocybin experienced phenomena which were indistinguishable from, if not identical with . . . the categories defined by our typology of mysticism."¹¹

⁹ *Ibid.*, pp. 45, 46.

¹⁰ Timothy Leary, "The Religious Experience: Its Production and Interpretation," *The Psychedelic Review*, 1, 3 (1964): 325.

¹¹ "Drugs and Mysticism: An Analysis of the Relationship between Psychedelic Drugs and the Mystical Consciousness," a thesis presented to the Committee on Higher Degrees in History and Philosophy of Religion, Harvard University, June 1963.

With the thought that the reader might like to test his own powers of discernment on the question being considered, I insert here a simple test I gave to a group of Princeton students following a recent discussion sponsored by the Woodrow Wilson Society:

Below are accounts of two religious experiences. One occurred under the influence of drugs, one without their influence. Check the one you think *was* drug-induced.

I

Suddenly I burst into a vast, new, indescribably wonderful universe. Although I am writing this over a year later, the thrill of the surprise and amazement, the awesomeness of the revelation, the engulfment in an overwhelming feeling-wave of gratitude and blessed wonderment, are as fresh, and the memory of the experience is as vivid, as if it had happened five minutes ago. And yet to concoct anything by way of description that would even hint at the magnitude, the sense of ultimate reality . . . this seems such an impossible task. The knowledge which has infused and affected every aspect of my life came instantaneously and with such complete force of certainty that it was impossible, then or since, to doubt its validity.

II

All at once, without warning of any kind, I found myself wrapped in a flame-colored cloud. For an instant I thought of fire . . . the next, I knew that the fire was within myself. Directly afterward there came upon me a sense of exultation, of immense joyousness accompanied or immediately followed by an intellectual illumination impossible to describe. Among other things, I did not merely come to believe, but I saw that the universe is not composed of dead matter, but is, on the contrary, a living Presence; I became conscious in myself of eternal life. . . . I saw that all men are immortal: that the cosmic order is such that without any preadventure all things work together for the good of each and all; that the foundation principle of the world . . . is what we call love, and that the happiness of each and all is in the long run absolutely certain.

On the occasion referred to, twice as many students (46) answered incorrectly as answered correctly (23). I bury the correct answer in a footnote to preserve the reader's opportunity to test himself.¹²

Why, in the face of this considerable evidence, does Zaehner hold that drug experiences cannot be authentically religious? There appear to be three reasons:

¹² The first account is quoted anonymously in "The Issue of the Consciousness-expending Drugs," *Main Currents in Modern Thought*, 20, 1 (September-October, 1963): 10-11. The second experience was that of Dr. R. M. Bucke, the author of *Cosmic Consciousness*, as quoted in William James, *The Varieties of Religious Experience* (New York: Modern Library, 1902), pp. 390-391. The former experience occurred under the influence of drugs; the latter did not.

1. His own experience was "utterly trivial." This of course proves that not all drug experiences are religious; it does not prove that no drug experiences are religious.

2. He thinks the experiences of others that appear religious to them are not truly so. Zaehner distinguishes three kinds of mysticism: nature mysticism, in which the soul is united with the natural world; monistic mysticism, in which the soul merges with an impersonal absolute; and theism, in which the soul confronts the living, personal God. He concedes that drugs can induce the first two species of mysticism, but not its supreme instance, the theistic. As proof, he analyzes Huxley's experience as recounted in *The Doors of Perception* to show that it produced at best a blend of nature and monistic mysticism. Even if we were to accept Zaehner's evaluation of the three forms of mysticism, Huxley's case, and indeed Zaehner's entire book, would prove only that not every mystical experience induced by the drugs is theistic. Insofar as Zaehner goes beyond this to imply that drugs do not and cannot induce theistic mysticism, he not only goes beyond the evidence but proceeds in the face of it. James Slotkin reports that the peyote Indians "see visions, which may be of Christ Himself. Sometimes they hear the voice of the Great Spirit. Sometimes they become aware of the presence of God and of those personal shortcomings which must be corrected if they are to do His will."¹³ And G. M. Carstairs, reporting on the use of psychedelic *bhang* in India, quotes a Brahmin as saying, "It gives good bhakti. . . . You get a very good bhakti with bhang," *bhakti* being precisely Hinduism's theistic variant.¹⁴

3. There is a third reason why Zaehner might doubt that drugs can induce genuinely mystical experiences. Zaehner is a Roman Catholic, and Roman Catholic doctrine teaches that mystical rapture is a gift of grace and as such can never be reduced to man's control. This may be true; certainly the empirical evidence cited does not preclude the possibility of a genuine ontological or theological difference between natural and drug-induced religious experiences. At this point, however, we are considering phenomenology rather than ontology, description rather than interpretation, and on this level there is no difference. Descriptively, drug experiences cannot be distinguished from their natural religious counterpart. When the current philosophical authority on mysticism, W. T. Stace, was asked whether the drug experience is similar

¹³ James S. Slotkin, *Peyote Religion* (New York: Free Press of Glencoe, 1956).

¹⁴ "Daru and Bhang," *Quarterly Journal of the Study of Alcohol*, 15 (1954): 229.

to the mystical experience, he answered, "It's not a matter of its being *similar* to mystical experience; it is mystical experience."

What we seem to be witnessing in Zaehner's *Mysticism Sacred and Profane* is a reenactment of the age-old pattern in the conflict between science and religion. Whenever a new controversy arises, religion's first impulse is to deny the disturbing evidence science has produced. Seen in perspective, Zaehner's refusal to admit that drugs can induce experiences descriptively indistinguishable from those which are spontaneously religious is the current counterpart of the seventeenth-century theologians' refusal to look through Galileo's telescope or, when they did, their persistence on dismissing what they saw as machinations of the devil. When the fact that drugs can trigger religious experiences becomes incontrovertible, discussion will move to the more difficult question of how this new fact is to be interpreted. The latter question leads beyond phenomenology into philosophy.

3. *Drugs and Religion Viewed Philosophically*

Why do people reject evidence? Because they find it threatening, we may suppose. Theologians are not the only professionals to utilize this mode of defense. In his *Personal Knowledge*,¹⁵ Michael Polanyi recounts the way the medical profession ignored such palpable facts as the painless amputation of human limbs, performed before their own eyes in hundreds of successive cases, concluding that the subjects were imposters who were either deluding their physicians or colluding with them. One physician, Esdaile, carried out about 300 major operations painlessly under mesmeric trance in India, but neither in India nor in Great Britain could he get medical journals to print accounts of his work. Polanyi attributes this closed-mindedness to "lack of a conceptual framework in which their discoveries could be separated from specious and untenable admixtures."

The "untenable admixture" in the fact that psychotomimetic drugs can induce religious experience is its apparent implicate: that religious disclosures are no more veridical than psychotic ones. For religious skeptics, this conclusion is obviously not untenable at all; it fits in beautifully with their thesis that *all* religion is at heart an escape from reality. Psychotics avoid reality by retiring into dream worlds of make-believe; what better evidence that religious visionaries do the same than the fact that identical changes in brain chemistry produce both states of mind?

¹⁵ Chicago: Univ. of Chicago Press, 1958.

Had not Marx already warned us that religion is the "opiate" of the people?—apparently he was more literally accurate than he supposed. Freud was likewise too mild. He "never doubted that religious phenomena are to be understood only on the model of the neurotic symptoms of the individual."¹⁶ He should have said "psychotic symptoms."

So the religious skeptic is likely to reason. What about the religious believer? Convinced that religious experiences are not fundamentally delusory, can he admit that psychotomimetic drugs can occasion them? To do so he needs (to return to Polanyi's words) "a conceptual framework in which [the discoveries can] be separated from specious and untenable admixtures," the "untenable admixture" being in this case the conclusion that religious experiences are in general delusory.

One way to effect the separation would be to argue that, despite phenomenological similarities between natural and drug-induced religious experiences, they are separated by a crucial *ontological* difference. Such an argument would follow the pattern of theologians who argue for the "real presence" of Christ's body and blood in the bread and wine of the Eucharist despite their admission that chemical analysis, confined as it is to the level of "accidents" rather than "essences," would not disclose this presence. But this distinction will not appeal to many today, for it turns on an essence-accident metaphysics which is not widely accepted. Instead of fighting a rear-guard action by insisting that if drug and non-drug religious experiences cannot be distinguished empirically there must be some transempirical factor that distinguishes them and renders the drug experience profane, I wish to explore the possibility of accepting drug-induced experiences as religious without relinquishing confidence in the truth-claims of religious experience generally.

To begin with the weakest of all arguments, the argument from authority: William James did not discount *his* insights that occurred while his brain chemistry was altered. The paragraph in which he retrospectively evaluates his nitrous oxide experiences has become classic, but it is so pertinent to the present discussion that it merits quoting once again.

One conclusion was forced upon my mind at that time, and my impression of its truth has ever since remained unshaken. It is that our normal waking consciousness, rational consciousness as we call it, is but one special type of consciousness, whilst all about it, parted from it by the filmiest of screens, there lie potential forms of consciousness entirely different. We may go

¹⁶ *Totem and Taboo* (New York: Modern Library, 1938).

through life without suspecting their existence; but apply the requisite stimulus, and at a touch they are there in all their completeness, definite types of mentality which probably somewhere have their field of application and adaptation. No account of the universe in its totality can be final which leaves these other forms of consciousness quite disregarded. How to regard them is the question—for they are so discontinuous with ordinary consciousness. Yet they may determine attitudes though they cannot furnish formulas, and open a region though they fail to give a map. At any rate, they forbid a premature closing of our accounts with reality. Looking back on my own experiences, they all converge toward a kind of insight to which I cannot help ascribing some metaphysical significance (*op. cit.*, 378–379).

To this argument from authority, I add two arguments that try to provide something by ways of reasons. Drug experiences that assume a religious cast tend to have fearful and/or beatific features, and each of my hypotheses relates to one of these aspects of the experience.

Beginning with the ominous, “fear of the Lord,” awe-ful features, Gordon Wasson, the New York banker-turned-mycologist, describes these as he encountered them in his psilocybin experience as follows: “Ecstasy! In common parlance . . . ecstasy is fun. . . . But ecstasy is not fun. Your very soul is seized and shaken until it tingles. After all, who will choose to feel undiluted awe? . . . The unknowing vulgar abuse the word; we must recapture its full and terrifying sense.”¹⁷ Emotionally the drug experience can be like having forty-foot waves crash over you for several hours while you cling desperately to a life-raft which may be swept from under you at any minute. It seems quite possible that such an ordeal, like any experience of a close call, could awaken rather fundamental sentiments respecting life and death and destiny and trigger the “no atheists in foxholes” effect. Similarly, as the subject emerges from the trauma and realizes that he is not going to be insane as he had feared, there may come over him an intensified appreciation like that frequently reported by patients recovering from critical illness. “It happened on the day when my bed was pushed out of doors to the open gallery of the hospital,” reads one such report:

I cannot now recall whether the revelation came suddenly or gradually; I only remember finding myself in the very midst of those wonderful moments, beholding life for the first time in all its young intoxication of loveliness, in its unspeakable joy, beauty, and importance. I cannot say exactly what the mysterious change was. I saw no new thing, but I saw all the usual things in a miraculous new light—in what I believe is their true light. I

¹⁷ “The Hallucinogenic Fungi of Mexico: An Inquiry into the Origins of the Religious Idea among Primitive Peoples,” *Harvard Botanical Museum Leaflets*, 19, 7 (1961).

saw for the first time how wildly beautiful and joyous, beyond any words of mine to describe, is the whole of life. Every human being moving across that porch, every sparrow that flew, every branch tossing in the wind, was caught in and was a part of the whole mad ecstasy of loveliness, of joy, of importance, of intoxication of life.¹⁸

If we do not discount religious intuitions because they are prompted by battlefields and *physical* crises; if we regard the latter as "calling us to our senses" more often than they seduce us into delusions, need comparable intuitions be discounted simply because the crises that trigger them are of an inner, *psychic* variety?

Turning from the hellish to the heavenly aspects of the drug experience, *some* of the latter may be explainable by the hypothesis just stated; that is, they may be occasioned by the relief that attends the sense of escape from high danger. But this hypothesis cannot possibly account for *all* the beatific episodes, for the simple reason that the positive episodes often come first, or to persons who experience no negative episodes whatever. Dr. Sanford Unger of the National Institute of Mental Health reports that among his subjects "50 to 60% will not manifest any real disturbance worthy of discussion," yet "around 75% will have at least one episode in which exaltation, rapture, and joy are the key descriptions."¹⁹ How are we to account for the drug's capacity to induce peak experiences, such as the following, which are *not* preceded by fear?

A feeling of great peace and contentment seemed to flow through my entire body. All sound ceased and I seemed to be floating in a great, very very still void or hemisphere. It is impossible to describe the overpowering feeling of peace, contentment, and being a part of goodness itself that I felt. I could feel my body dissolving and actually becoming a part of the goodness and peace that was all around me. Words can't describe this. I feel an awe and wonder that such a feeling could have occurred to me.²⁰

Consider the following line of argument. Like every other form of life, man's nature has become distinctive through specialization. Man has specialized in developing a cerebral cortex. The analytic powers of this instrument are a standing wonder, but the instrument seems less able to provide man with the sense that he is meaningfully related to his environment: to life, the world, and history in their wholeness. As Albert Camus describes the situa-

¹⁸ Margaret Prescott Montague, *Twenty Minutes of Reality* (St. Paul, Minn.: Macalester Park, 1947), pp. 15, 17.

¹⁹ "The Current Scientific Status of Psychedelic Drug Research," read at the Conference on Methods in Philosophy and the Sciences, New School for Social Research, May 3, 1964, and scheduled for publication in David Solomon, ed., *The Conscious Expanders* (New York: Putnam, fall of 1964).

²⁰ Quoted by Dr. Unger in the paper just mentioned.

tion, "If I were . . . a cat among animals, this life would have a meaning, or rather this problem would not arise, for I should belong to this world. I would *be* this world to which I am now opposed by my whole consciousness."²¹ Note that it is Camus' consciousness that opposes him to his world. The drugs do not knock this consciousness out, but while they leave it operative they also activate areas of the brain that normally lie below its threshold of awareness. One of the clearest objective signs that the drugs are taking effect is the dilation they produce in the pupils of the eyes, and one of the most predictable subjective signs is the intensification of visual perception. Both of these responses are controlled by portions of the brain that lie deep, further to the rear than the mechanisms that govern consciousness. Meanwhile we know that the human organism is interlaced with its world in innumerable ways it normally cannot sense—through gravitational fields, body respiration, and the like: the list could be multiplied until man's skin began to seem more like a thoroughfare than a boundary. Perhaps the deeper regions of the brain which evolved earlier and are more like those of the lower animals—"If I were . . . a cat . . . I should belong to this world"—can sense this relatedness better than can the cerebral cortex which now dominates our awareness. If so, when the drugs rearrange the neurohumors that chemically transmit impulses across synapses between neurons, man's consciousness and his submerged, intuitive, ecological awareness might for a spell become interlaced. This is, of course, no more than a hypothesis, but how else are we to account for the extraordinary incidence under the drugs of that kind of insight the keynote of which James described as "invariably a reconciliation"? "It is as if the opposites of the world, whose contradictoriness and conflict make all our difficulties and troubles, were melted into one and the same genus, but *one of the species*, the nobler and better one, *is itself the genus, and so soaks up and absorbs its opposites into itself*" (*op. cit.*, 379).

4. *The Drugs and Religion Viewed "Religiously"*

Suppose that drugs can induce experiences indistinguishable from religious experiences and that we can respect their reports. Do they shed any light, not (we now ask) on life, but on the nature of the religious life?

One thing they may do is throw religious experience itself into perspective by clarifying its relation to the religious life as a whole. Drugs appear able to induce religious experiences; it is

²¹ *The Myth of Sisyphus* (New York: Vintage, 1955), p. 38.

less evident that they can produce religious lives. It follows that religion is more than religious experiences. This is hardly news, but it may be a useful reminder, especially to those who incline toward "the religion of religious experience"; which is to say toward lives bent on the acquisition of desired states of experience irrespective of their relation to life's other demands and components.

Despite the dangers of faculty psychology, it remains useful to regard man as having a mind, a will, and feelings. One of the lessons of religious history is that, to be adequate, a faith must rouse and involve all three components of man's nature. Religions of reason grow arid; religions of duty, leaden. Religions of experience have their comparable pitfalls, as evidenced by Taoism's struggle (not always successful) to keep from degenerating into quietism, and the vehemence with which Zen Buddhism has insisted that once students have attained *satori*, they must be driven out of it, back into the world. The case of Zen is especially pertinent here, for it pivots on an enlightenment experience—*satori*, or *kensho*—which some (but not all) Zennists say resembles LSD. Alike or different, the point is that Zen recognizes that unless the experience is joined to discipline, it will come to naught:

Even the Buddha . . . had to sit. . . . Without *joriki*, the particular power developed through *zazen* [seated meditation], the vision of oneness attained in enlightenment . . . in time becomes clouded and eventually fades into a pleasant memory instead of remaining an omnipresent reality shaping our daily life. . . . To be able to live in accordance with what the Mind's eye has revealed through *satori* requires, like the purification of character and the development of personality, a ripening period of *zazen*.²²

If the religion of religious experience is a snare and a delusion, it follows that no religion that fixes its faith primarily in substances that induce religious experiences can be expected to come to a good end. What promised to be a short cut will prove to be a short circuit; what began as a religion will end as a religion surrogate. Whether chemical substances can be helpful *adjuncts* to faith is another question. The peyote-using Native American Church seems to indicate that they can be; anthropologists give this church a good report, noting among other things that members resist alcohol and alcoholism better than do nonmembers.²³ The conclusion to which evidence currently points would seem to be that chemicals *can* aid the religious life, but only where set within a context of faith (meaning by this the conviction that what they

²² Philip Kapleau, *Zen Practice and Attainment*, a manuscript in process of publication.

²³ Slotkin, *op. cit.*

disclose is true) and discipline (meaning diligent exercise of the will in the attempt to work out the implications of the disclosures for the living of life in the everyday, common-sense world).

Nowhere today in Western civilization are these two conditions jointly fulfilled. Churches lack faith in the sense just mentioned; hipsters lack discipline. This might lead us to forget about the drugs, were it not for one fact: the distinctive religious emotion and the emotion that drugs unquestionably can occasion—Otto's *mysterium tremendum, majestas, mysterium fascinans*; in a phrase, the phenomenon of religious awe—seems to be declining sharply. As Paul Tillich said in an address to the Hillel Society at Harvard several years ago:

The question our century puts before us [is]: Is it possible to regain the lost dimension, the encounter with the Holy, the dimension which cuts through the world of subjectivity and objectivity and goes down to that which is not world but is the mystery of the Ground of Being?

Tillich may be right; this may be the religious question of our century. For if (as we have insisted) religion cannot be equated with religious experiences, neither can it long survive their absence.

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AN EMPIRICAL BASIS FOR PSYCHOLOGICAL EGOISM

IT is commonly believed in the philosophical world today that the age-old problem of psychological egoism is merely a pseudo-problem and that this is true just because the a priori philosophical arguments that have traditionally been given in favor of egoism depend in the main upon confusions about the logic of our ordinary language. It has been claimed, for example, that the well-known argument that we act selfishly even when we want to help others because in such cases we are still attempting to *satisfy our own desire* to help others, is fallaciously generated by misunderstandings of the proper use of terms like 'want', 'satisfy', and 'desire'.¹

In *Butler's Moral Philosophy*, Austin Duncan-Jones, expressing Butler's view, and, it seems from the context, his own as well, states that if there is something wrong with all the a priori philosophical arguments that have traditionally been given in favor of egoism (which he has earlier identified with the doctrine that

¹ See P. H. Nowell-Smith's *Ethics* (New York: Philosophical Library, 1957), ch. 10, *passim*.

all human acts are selfish),² then there is little else to recommend the theory, since "the appearance of things, undistorted by theory," is that men sometimes do act unselfishly, disinterestedly. Only one who already believed in the validity of the philosophical arguments for egoism would have any reason to interpret the facts of human behavior in a way compatible with the doctrine of egoism.³ Thus Duncan-Jones seems clearly to be ruling out the possibility that the (empirical) facts as they stand could, with any semblance of objectivity, be used to support egoism. And many other contemporary philosophers would, I think, tend to agree with him.

In the present paper I wish to argue that psychological egoism may well have a basis in the empirical facts of human psychology. Certain contemporary learning theorists, e.g., Hull and Skinner, have put forward behavioristic theories of the origin and functioning of human motives which posit a certain number of basically "selfish," unlearned primary drives or motives (like hunger, thirst, sleep, elimination, and sex), explain all other, higher-order, drives or motives as derived genetically from the primary ones via certain "laws of reinforcement," and, further, deny the "functional autonomy" of those higher-order drives or motives.⁴ Now it is a hotly debated issue in contemporary Learning Theory whether any theory such as we have described briefly above could adequately explain adult human behavior. I shall, however, argue only that a theory of the above kind may well be true, and that from such a theory, fortified only by one additional psychological premise, the truth of egoism (non-altruism) logically follows. I hope to show, thereby, that the question of psychological egoism is still an open empirical issue, however fallacious be the *philosophical* arguments for it.

But what is "functional autonomy," and how does the lack of it help to show our actions to be selfish? According to behavior-

² It has been suggested to me by P. R. Foot that only those of one's acts which are somehow related to the wants or interests of others can correctly be called either selfish or unselfish. If this be so, then Duncan-Jones' definition of egoism will make that doctrine trivially false, just because there are some human actions that are neither selfish nor unselfish. In order to avoid such an eventuality, I shall mean by *egoism* the slightly different thesis (perhaps more accurately, but clumsily, designated *non-altruism*) that no human act is ever *unselfish*.

³ See p. 109 of *Butler's Moral Philosophy*.

⁴ It will not, I think, be necessary for my purposes to be truer to ordinary language or more precise with the concepts of drive and motive than are the learning theorists themselves. Thus, e.g., I shall be using 'drive' and 'motive' interchangeably in this paper.

istic learning psychologists a higher-order (acquired) motive is functionally autonomous when it becomes causally independent of primary motives (especially of those motives association with which enabled it to be acquired in the first place) in such a way that one will indefinitely keep acting from that motive, even if rewards for those other, primary, motives are no longer in general associated with such action.⁵ A higher-order drive or motive is *not* functionally autonomous, i.e., is functionally dependent, if and only if when we cut off all reinforcement of it by primary rewards (rewards of primary drives) and there are, in addition, both a sufficient number of "extinction trials" (occurrences of acts done from that higher-order motive which are not associated even indirectly, i.e., through other higher-order motives, with primary rewards) and a complete absence, during those extinction trials, of primary rewards for any similar higher-order motives (to eliminate the possibility of generalization of primary rewards from motives other than that being extinguished), the higher-order drive or motive actually does extinguish; i.e., the person whose higher-order motive is being extinguished eventually, even if perhaps only very gradually, ceases to act from that higher-order motive.

It is necessary for a motive *derived genetically* from "selfish" (or at least not *unselfish*) primary drives also to be *functionally dependent* upon them if we are to be able to say that acts performed from that motive are never unselfish. For the fact that in the past we performed such acts only because they led to the satisfaction of some other non-unselfish motive or motives, i.e., because they were reinforced by primary-drive rewards, does not show that such acts performed *now* are not unselfish. To argue thus would be indeed to commit a "genetic fallacy." An act must *presently* be causally connected with drives that are not unselfish in order to be considered selfish. Now those who deny functional autonomy are saying in effect that whenever, e.g., one acts benevolently (i.e., from what the psychologist would call the higher-order motive of benevolence), one would not be performing that act, or, at least, would not indefinitely continue to perform acts of that kind, if such benevolent action were not in general still associated with and reinforced by the satisfaction of such non-unselfish primary drives as hunger and thirst, whether those drives be the

⁵ I have given a very brief account of the notions of primary and higher-orders drives or motives; but it should be sufficient for the purposes of this paper. A more complete account of these and of the other psychological notions I make use of can be found in practically any textbook of experimental psychology.

same as or different from the primary drives from which the motive of benevolence actually originated.

I do not, however, wish to maintain that the hypothesis of functional dependence (together with its learning-theoretical underpinnings) entails egoism all by itself. The hypothesis does, indeed, entail that we would never continue to act "benevolently" or "self-sacrificingly" if such action on our part were not in general reinforced by the rewarding of selfish primary drives. But is it not possible that the primary rewards received, in general, when one acts benevolently or self-sacrificingly are not so great as those relinquished in the doing of such acts? It might, in other words, be the case that a poverty-stricken mother who sacrificed some of her own food so that her child might eat better would not act in this way unless she were, in general, receiving (however indirectly) some primary-drive satisfactions for her sacrifices. And yet we would still call her actions unselfish if we thought that the rewards she was sacrificing (reduction of her hunger) were greater than those she was getting in return; for is not the habit of giving more than one asks in return an exemplary case of unselfishness?

I should like now to show that a certain empirical hypothesis may, when taken together with the hypothesis of functional dependence, entail the thesis of psychological egoism and rule out the possibility of a case like the above, even if the hypothesis of functional dependence taken alone does not.⁶

Let us imagine that we have a method for determining empirically which primary rewards a person prefers to which others. We set up various situations where the man has to choose between primary rewards, situations involving no moral factors and no interests of other people, and determine the man's preferences. A learning theorist might claim that it is true as a matter of empirical fact that whenever a man systematically (i.e., as a general rule) continues to sacrifice primary reward x to other people, he does so only because he usually obtains thereby some primary reward y and because y ranks higher than x on the person's preference scorecard, as determined in situations where no considerations of other people's interests and thus of sacrifice to other people's interests were involved.⁷ And the above empirical claim, which involves, but is not exhausted by, the claim that functional de-

⁶ I am again indebted to Mrs. Foot for the insight that the hypothesis of functional dependence does not itself, alone, entail that no act is unselfish. In addition, I am indebted to discussion with Prof. R. P. Wolff for some of the points I shall be making hereafter.

⁷ Of course, there are some primary rewards, like sexual gratification, that are very hard to measure in isolation from all moral considerations.

pendence is true, entails, I think, the thesis of psychological egoism. For if our conscious acts of benevolence and sympathy and sacrifice, etc., would eventually cease to be performed by us if we did not, in performing those acts, in general give away *less* in the way of primary-drive satisfactions than we actually got in return, the inevitable conclusion would be that all our acts were fundamentally (or ultimately or "really") motivated by our "selfish" primary drives. And it would be accurate, if this were the case, to say that the driving forces behind all our so-called higher actions were "selfish" primary motives, since only those higher actions would continue to be performed which usually led to a net gain of primary-reward satisfaction for the individuals performing them. If this were the case, then, indeed, none of our actions would ever "really" or fundamentally or ultimately be unselfish. The above-described case of the mother who gives away more in the way of primary-drive satisfaction than she gets for herself in return would just never come up. Psychological egoism would be true.

We have thus shown that egoism (in our sense) would be true if certain psychological hypotheses turned out to be true and that the question of the truth of psychological egoism is an empirical question. But that is not to say that contemporary psychology has been able to prove the truth of these hypotheses or that psychologists are even all agreed that, with the further advancement of psychology as a science, these hypotheses will as a matter of fact be verified. There are many psychologists who think, for example, that some higher-order drives *do* become functionally autonomous. Gordon Allport, for instance, has brought to light a good deal of psychological evidence in favor of this contention.⁸ Furthermore, the hypothesis of functional dependence is very difficult to establish experimentally, for reasons well known to psychologists. In the words of Neal Miller, "a strong learned drive may seem unaffected for many [extinction] trials and still eventually extinguish. When generalization, higher-order reinforcement, and shifts from one reinforcing agent to another are added to this possibility, it can be seen how difficult it is in complex human situations to determine whether a habit [drive] actually is functionally autonomous."⁹ In other words, even if there is no functional autonomy, there are many ways in which

⁸ See his *Personality: a Psychological Interpretation*.

⁹ "Learnable Drives and Rewards," in S. S. Stevens, ed., *Handbook of Experimental Psychology*, p. 469.

See also D. C. McClelland, "Functional Autonomy of Motives as an Extinction Phenomenon," *Psychological Review*, 49 (1942): 272-283.

a functionally dependent drive might *appear* to be autonomous, because of distorting psychological factors that can never with absolute certainty be ruled out in the context of human motivation. However, the question whether some drive is autonomous is still empirical in principle, however difficult it may be in practice, given the current rudimentary state of the science of psychology, to determine whether that drive would extinguish if its association with primary reinforcements were entirely severed.

Consider also the hypothesis that, together with the hypothesis of functional dependence, entails egoism, the hypothesis, namely, that even if people sometimes do sacrifice and continue to sacrifice a certain kind of primary reward, they never do so unless they in general get some greater primary reward in return. This hypothesis, I think, might also turn out to be empirically false. It might turn out, for example, that, although people never made sacrifices unless they got something in return, they sometimes sacrificed some primary reward x for some other primary reward y even though y ranked lower on their preference scorecard as determined in morally neutral circumstances. In such a case there are two things we can say. We might well say that the scorecard as determined in morally neutral circumstances does not tell us a man's real preferences, for if he prefers x to y in neutral circumstances, but prefers y to x when certain other people's interests are involved, who can say what his real preference is? We might, on the other hand, want to say that what a man prefers in morally neutral circumstances really does tell us fairly accurately what he *really* prefers, so that if his preferences differ where moral considerations are involved, we have a right to say that the man has, in the interests of morality, gone against his own preferences and made an unselfish sacrifice. Whether we should want to say the first or the second of these things would depend a great deal, I think, on a number of other scientifically relevant factors. Anyone who would *in principle* refuse to say the second kind of thing would in effect be considering it to be tautologously true that men do not persist in acting against their own self-interest; that is, he would be making the thesis of egoism into a mere tautology, which, I prefer to think, it is not. It seems that there very well could be circumstances in which it would from a scientific point of view be advisable to say that a man had acted against his own real preferences, had persistently sacrificed a greater for a smaller primary reward out of a sense of duty or a feeling of benevolence. Such circumstances might exist, for example, if we had a detailed knowledge of brain physiology which showed that the brain contained a "preference" center and a "morality" center and that the moral-

ity center affected our actions not by directly influencing the preference center, but, rather, by acting as an inhibitor or as a modifier on certain motor impulses sent out by the preference center. Such a physiological theory would make it eminently plausible, I think, to believe that what we did in moral contexts might consistently go against our real preferences. In terms of such a theory, then, it might be possible empirically to refute the hypothesis that we never consistently or systematically sacrifice the greater for the smaller primary reward. Thus it would seem that both parts of that psychological theory which, I have claimed, entails psychological egoism, are open to empirical refutation, as well as confirmation.¹⁰

The psychological theory I have been describing should not be confused with a certain theory of human behavior and motivation put forward in recent years by the psychologist A. H. Maslow, a theory which, I believe, does *not* entail psychological egoism. According to Maslow people will not act from such higher motives as benevolence and love unless certain lower needs like hunger, safety, and elimination have already to some degree been satisfied. But once physiological and other lower needs are satisfied to a reasonable degree, needs to be benevolent, creative, loving, self-sacrificial, and the like will spring up of their own accord. And one will continue to act benevolently, creatively, etc., just as long as one's lower needs remain satisfied, even if none of one's benevolent or creative activities is actually reinforced by the satisfaction of lower needs (primary drives).¹¹

Clearly this theory differs substantially from the one I have been describing. For it does *not* assert that we will persist in acting benevolently, etc., only if such acts are in general associated with the satisfaction of selfish primary drives. It says merely that we require a certain amount of primary-drive contentment if we are to become people who constantly act benevolently *whether we are rewarded for doing so or not*. According to Maslow, a man often will habitually act from benevolence even though there is "nothing in it for him." It is clear, then, that his theory does *not* exclude the possibility of unselfish human action. The theory *we* have been discussing, on the other hand, does exclude that possibility, just because it implies that we never persist in performing any kind of action unless there is in general something in it for us.

¹⁰ There is indeed still another way we have not yet mentioned in which this psychological theory might empirically be refuted, namely, if some theory (like Hume's) which made benevolence or other unselfish motives into basic human instincts (primary drives) turned out to be correct.

¹¹ See Maslow's *Motivation and Personality*, ch. 5, *passim*.

It would seem, then, that, as psychology stands today, there is at least some reason to think that the psychological theory we have been discussing may be true. Consequently, the truth of psychological egoism is still an open empirical question. Duncan-Jones and others are mistaken in their belief that, now that the *a priori* arguments for egoism seem to have been shown to be fallacious, no further case can possibly be made in its favor. Perhaps the only reason philosophers are thus mistaken is their ignorance of contemporary Learning Theory, its issues, and its results.¹² It is interesting to note, furthermore, that it is impossible to object to an empirical argument for egoism, the way one so often objects to *a priori* arguments for egoism, by saying that such arguments end up depriving 'selfish' of the logical possibility of a contrast, thus rendering the word meaningless. For egoism will be false if either part of the psychological theory we have been discussing is false. Thus, in making the truth of egoism depend on the truth of an empirically falsifiable psychological theory, I am leaving open at the very least the *logical* possibility that egoism is false, that some acts are unselfish.

I might add, finally, that the explanation I have attempted to give of the possibility of arguing on an empirical basis for egoism may help us to understand why so many people, especially beginning students of philosophy, are so dissatisfied by attempts to discredit egoism by showing the invalidity of the traditional philosophical arguments that have been put forward to prove it, and why the doctrine of egoism keeps cropping up, however many be the philosophical voices that seek to silence it. I am willing to conjecture that egoism will not lie dead, because people in some way see that there may be more in favor of egoism than *a priori* arguments. It is my very tentative suggestion that the reason for this may be that even those with little or no training in psychology believe, however inarticulately, that something like the psychological theory we have been discussing in this paper may well be true, believe that men who act consistently in a benevolent manner, for example, would not be acting benevolently unless their selfish desires and/or interests were usually satisfied by their doing so.

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¹² I do not, however, wish to suggest that *psychologists* have been totally unaware of the philosophical consequences of their own theories. See, for example, Allport, *op. cit.*, p. 206.

BOOK REVIEW

Generalization in the Writing of History. LOUIS GOTTSCHALK, ed. Chicago: University of Chicago Press, 1963. xiii, 255 pp. (A Report of the Committee on Historical Analysis of the Social Science Research Council.) \$5.00.

Generalization in the Writing of History is the "report" of the third Committee on Historical Analysis of the Social Science Research Council, which functioned from 1956 to 1962, and it succeeds the famous Bulletins 54 (*Theory and Practice in Historical Study*, 1946) and 64 (*The Social Sciences in Historical Study*, 1954) published by the SSRC. Although these reports have been controversial and influential among historians and have been important occasions for reflection on the problems of historical knowledge by practicing historians, they have been little noticed by philosophers. On the other hand, the essays in the current volume show almost no acquaintance with philosophical discussions of historical knowledge and especially of historical explanation. The *locus classicus* of the current debate over historical explanation, Hempel's "The Function of General Laws in History," is not even mentioned; but then neither is Dilthey. There are frequent references in the volume to "philosophy," but almost always in the sense of "cosmic understanding of the course of human events past and to come" (113).

The contemporary situation in philosophy of history is in no significant way comparable to that in philosophy of physical science. Philosophers of science necessarily speak the language and share the theoretical sophistication of advanced science, and for their own part scientists are aware of and have made notable contributions to the logical analysis and philosophical foundations of theory, often as an integral part of their scientific activity. As a result, there is a community of philosophy of science in an even more specific and identifiable way than there is a "scientific community." But while historians have come to speak more and more of the historical "guild" (a term which suggests, among other things, a reluctance to decide whether it is a *gemeinschaft* or a *gesellschaft*), one of the things the present book unwittingly makes clear is that there is no community of philosophy of history. And this is surprising and noteworthy in view of the fact that the last two decades have seen a burgeoning of interest among both historians and philosophers in a subject once reserved by the former for presidential addresses to the American Historical Association and by the latter to continental metaphysicians. One

might suppose that there would be an interaction or at least an overlapping of interest between these two groups. But one would be wrong.

The differences among the three reports by historians raise more questions than they answer about this situation, but they constitute in themselves a chapter of recent intellectual history. Four of the six historians on the recent committee were connected with the original committee, formed in 1942, which produced Bulletin 54: Louis Gottschalk, chairman of this committee and editor of the present volume, was a member; Merle Curti was its chairman; Thomas C. Cochran was a member of both the first and second committees (and is the only one to have served on all three); and Roy F. Nichols, although not a member of the first committee, wrote the memorandum that led to its formation. But despite this extraordinary continuity of personnel, the three reports are very different from one another. They reflect a growing self-consciousness on the part of historians about the problems of theory of history, but reveal no developing consensus on how to deal with them.

Brooding over the tone and spirit of Bulletin 54 was the mitigated skepticism of Charles A. Beard; suffusing the second report was Ralph Turner's confident program of making history the science of comparative culture. The first report was cautious, inconclusive, even embarrassed. Reflecting primarily the awareness of the problem of historical relativism, it admitted that the historian cannot avoid the use of principles of "selection" and "interpretation" which are largely given by his "philosophy of life and of history"; and it recommended that historians should explicitly acknowledge such individual "presuppositions" (e.g., "socio-economic biases"). The philosophers Randall and Hook were extensively consulted by the committee, and chapters under their names appeared in the final report.

In general, Bulletin 54 seemed to be the last chapter in the history of the dissolution of the nineteenth-century school of "scientific" history, with its disavowal of speculation or interpretation in the establishment of "the facts." Bulletin 64, on the other hand, aspired to be the first chapter in the metamorphosis of history into a true social science. This was evident even in its form: unlike the first report, it was avowedly a "product of group thinking" in which no credit was given to the authors of individual chapters except for brief mention of "assignments" in the Foreword. Its theme was that history can and should become a "cumulative science" by the adoption of rigorous

methodology, including the "derivation" of empirically testable hypotheses in deductively formulated theory, the operational definition of concepts, and the application of technical tools such as projective testing techniques and the chi-square test for statistical significance. Collaborative group research, especially with other social scientists, was recommended. Moreover, the committee concluded, "analysis of values and of other philosophical ideas as they appear in historical data presents no special problem" (141); value judgments as *made* by historians were regarded as somewhat more problematical but not seriously so, because psychologists have "demonstrated empirically" that a set of interrelated hypotheses is the best check against unconscious bias, and because the committee believed in any case that "values eventually may be given more empirical status" (145). The second Committee had as consultants a distinguished roster of social scientists, to whom it owed its expertise on the logic of science. It did not consult with any philosophers.

By contrast, the report of the third committee significantly abandons the report form, apart from the indefatigable efforts of its editor in summarizing and cross-referencing. It appears as a collection of essays which do not as a rule refer to one another, adopt a uniform terminology, or debate common issues, although they were commissioned to fulfill a general plan for the collection. The book is divided into three parts. The third part is an extensive and well-organized bibliography of writings on historiography and philosophy of history which brings up to date the bibliography of Bulletin 54 (Bulletin 64 contained no bibliography). The second part consists of individual essays by members of the committee on the problem of historical generalization. The first part consists, with one exception, of essays by other historians asked by the committee to address themselves to the problem of generalization in their particular fields: two are by Ancient historians, two by Chinese historians, and three by Modern historians. Hans Meyerhoff served as philosophical consultant to the committee, and some of his pointed comments in letters to the committee have been included by the editor as footnotes.

The overwhelming impression produced by *Generalization in the Writing of History*, when it is read in the light of Bulletin 54 and Bulletin 64, is that history has reversed itself. In terms of the sense of linear and directional development—that informed sense of recognition by which historians of ideas and historians of art "place" an idea or artifact, in the absence of independent chronological evidence, by identifying it in a sequence of change for which earlier and later points are known—one would conclude,

I think, that the current volume belonged to a stage *between* Bulletins 54 and 64. From the standpoint of intellectual history, that is, the present volume is an anomaly, and raises a question which it does nothing itself to answer.

Some of the external marks of this anomaly have already been mentioned: (1) there is a high degree of continuity of personnel between the first and third committees, very little between either and the second; (2) the participation of philosophers is high in the first, reduced in the third, nonexistent in the second; (3) the first and third reports consist of individual essays, and include bibliographies which indicate an orientation toward the literature of the subject, whereas the second is a report of "group research" and dramatizes an orientation toward techniques by the omission of a bibliography.

But more significant than these formal marks is the drift of ideas. The present volume seems mainly directed toward eliminating the spell of the "particular and unique" by convincing historians that they cannot *avoid* generalization in any case, and that the attempt to do so results in the unconscious, unacknowledged, and hence uncritical use of generalizations. Every contribution to the volume makes this point in one way or another, and many attempt a classification of kinds of generalization (neatly summarized by the editor, pp. 195-203, 208-209). Yet at the same time, as Hans Meyerhoff pointed out (129 n.), the historians did not address themselves to the question whether these are characteristically and specifically *historical* generalizations, as distinguished from generalizations which belong to common-sense or to the methodologically refined social sciences such as psychology and sociology. To put it another way, the authors agree that historians must *use* some general statements, but they do not even raise the question whether there are specifically historical concepts and hypotheses or whether there is in any sense a logic of historical knowledge that is characteristically different from the logic of scientific knowledge which many philosophers and most social scientists have regarded as the model of knowledge in general.

In this respect, the present volume is an extended commentary on Proposition XVI of Bulletin 54: "Historians may formulate generalizations of limited validity which are useful in the interpretation of the past . . ." (138). But it does not go nearly so far in outlining and recommending a model of concept formation and a logic of confirmation and explanation as did Bulletin 64. The latter in effect adopted the thesis of the methodological unity of science; the present volume, like Bulletin 54, is indifferent

to the claims for, as well as to the criticisms of that thesis. In a sense, therefore, the historians whose labors led to this volume appear to have re-enacted the past without retracing their steps. It is unfortunate that in the end there is no answer to the question that led to the formation of the third Committee: "Whether the historian is at all competent from his own data and by his own methods to derive (*sic*) concepts that are neither so limited in scope as to be trivial nor so comprehensive as to be meaningless" (viii). The question remains unanswered because the meaning of "his own data" and "his own methods" is never elucidated. But at the same time, the historians have, in drawing back from the programmatic recommendations of Bulletin 64, unwittingly drawn closer to the position of recent philosophical criticisms, by Dray and others, of the "covering-law" model of historical explanation—criticisms which are at the same time arguments against the thesis of the methodological unity of science—and there is some hope that philosophers and historians may yet find common problems. Yet this has not occurred, although both groups independently have been trying to elucidate what is logically involved in "explaining" a past event. Perhaps some new problems would be useful, and the present volume at least suggests where they might be found. Even the authors of Bulletin 64 cautioned that, although "the analysis of interrelations goes on in all social science, the attempt to make a general synthesis of all major factors at work in a given conjuncture of events is peculiar to historical studies" (87). Now "synthesis" (sometimes called by historians "interpretative synthesis" or "narrative synthesis") is not a designation for everything that historians do, but it could be argued that it indicates an activity which is distinctive of history and for which the best claim for the autonomy of history can be made. Gottschalk distinguishes six "schools" of generalization in history (113–129; 206): (1) the school of the unique, which believes that history can and should avoid all generalization whatever; (2) the school of limited generalization, or narrative-descriptive history; (3) interpretative history, which goes beyond the "subject matter in hand" to indicate its interrelations with antecedent, simultaneous, and subsequent events; (4) comparative history, which draws analogies between its subject matter and other times and places; (5) nomothetic history, which seeks to confirm universally quantifiable generalizations; and (6) philosophy of history, i.e., "cosmic or panoramic ideologies or historical determinisms." It is obvious, however, that neither Gottschalk nor any of the other authors regard all of these schools as genuinely historical. They argue that the first is merely naive,

and include the last only as a history of astronomy might refer to astrology. Moreover, (2) seems to designate nothing which is not either (1) become conscious of itself nor a limiting case of (3); (5) is identical with sociology or social psychology; and (4) is clearly an uncritical version of (5). So (3) alone remains to include a "history," as in "a history of x ." And this is where history can claim a certain sovereignty, between the filing cabinets of the school of the unique on the one hand and the sociological theories of nomothetic social science on the other. But what are the criteria by which one interpretative synthesis can be preferred to another? How can one confirm a "synthesis" as distinguished from confirming a general statement? What is the structure of a history, as distinguished from the structure of the explanation of an historical fact? The historians who produced the present volume have not answered these questions, but they could justly reply that philosophers have not raised them.

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NEW ANTHOLOGIES, CLASSICAL AND CONTEMPORARY

Anthologies (Classics)

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